148 *Brit. J. Phil. Sci.* **18** (1967), 148–161 *Printed in Great Britain*

Reviews

L. Pearce Williams: *Michael Faraday: A Biography* (London, 1965, 70*s.*) Pp. xvi+531.

Given the underdeveloped state of historical research on nineteenth-century physical science, L. P. Williams's *Michael Faraday* is a notable achievement. Working virtually without relevant models, Professor Williams has assembled a massive and factually authoritative survey of Faraday's career from a scrupulous examination of published and unpublished sources, some previously unknown, most previously unexploited by scholars. Any future study of Faraday must start from this book, and no student of British science in the last century may ignore it. Nevertheless, whether as an analysis of Faraday's career or as a model for future research, the book has numerous and essential imperfections. Some of these may affect the future of nineteenth-century studies. In this place and at this comparatively late date, I shall therefore largely take for granted the work's great virtues as an authoritative source both of data and of problems for research and concentrate instead on those things which Williams has failed to do or has done badly.

As a portrait either of an individual or of a scientific career in nineteenth-century Britain, the book is disappointingly routine. Williams's predominant concern is with Faraday's scientific ideas and their philosophical background. Fewer than one-third of his pages are reserved for an account of the main extra-scientific events in Faraday's life together with brief essays on such topics as 'The Sandemanian Church', 'The Royal Institution', and 'Faraday, the Educator'. Those pages add little except reliability to past accounts. Given the nature of Faraday's achievements and of Williams's interests, the distribution of space may well be appropriate. But it is unfortunately not just a distribution but a division. The two parts of Williams's narrative seldom interpenetrate or illuminate each other. What results is close to being two separate books.

Of course Faraday was a giant only in his science, but he came from what was, in Britain, representative stock. As a self-educated scientist and as a member of a dissenting sect (perhaps also as a married but childless physicist), Faraday belonged to groups which were disproportionately represented in British science during the first half of the nineteenth century. The Royal Institution, with which his entire scientific career was associated, was also a uniquely British organisation. Its foundation, its form, and its ultimate success are all symptomatic of forces which, during Faraday's lifetime, produced an almost total reorganisation of British scientific institutions and education. As incidental facts about Faraday, Williams supplies all these bits of information, but readers who see them as parts of a British pattern, unimaginable in France or Germany, must supply that viewpoint for themselves. Those able to do so will, I think, be disconcerted to discover how little this full-scale biographical study adds to their understanding of scientific life in Faraday's nation and time.

Nor is Williams's portrayal of Faraday, the man, more successful. This biography supplies many of the requisite fragments of a personality but not a human being plausible to the twentieth-century reader. Probably Williams is right to refuse (p. 132, n. 15) to speculate about the *causes* of Faraday's 'giddiness, loss of memory, and headaches throughout most of his adult life', but he might have supplied some sense of what life and work were like for the scientist who suffered from these disabilities. For him they are part of the chronology but not of the man. Thus he writes on page 102: 'The headaches were to increase in severity . . . as the years went by. The forced vacation in 1828 was to be repeated many times. The pattern of his life was formed by the alternation of work and rest'. Four pages later, however, Williams continues: 'Secure in his faith and beloved by his wife and relatives he was able to devote his entire energy for forty years to the task of uncovering one corner of [the truth].' Can this representative bit of rhetoric be about the man whose disabilities Williams has just chronicled?

Similar problems of reconciliation arise elsewhere. For example, the Faraday who 'was guided only by the love of truth' (p. 106) never acquires the complexity to be also the man who was repeatedly concerned to establish his priority in scientific discovery. As a result, one repeatedly wonders whether to take Williams seriously on those occasions when he does supply his subject's innermost thoughts: 'But, inside, [Faraday] must have wondered if he were not in error; if the simplicity and consonance of his theory with observed phenomena were not merely an illusion buttressed by his own ignorance' (p. 18). And, beyond this, there are many points of information for which the reader will search in vain. Which of the mature Faraday's colleagues and correspondents were friends, which acquaintances? What, in a personal or a scientific way, did he draw from them? Williams excerpts many letters, some of which would surely illuminate points of this sort. But the reader must still guess at answers if he is to make Faraday comprehensible as a man. Because they usually quoted at greater length, the nineteenth-century British 'lives', on which the biographical components of Williams's narrative seem modelled, often inadvertently provided more clues.

Probably it was a mistake to attempt the full biographical mode. Williams's main objective, which occupies most of the book, is an account of the development of Faraday's experiments and ideas, based principally on the published *Diaries* and *Experimental Researches*. Here he has been vastly more successful and might have been still more so in the absence of the concern with biography. With his attention focused on his central character, Williams sometimes fails to catch the relevant essentials of work done by Faraday's predecessors, contemporaries, and successors. Nevertheless, this aspect of Williams's work richly merits the prize it has recently been awarded by the U.S. History of Science Society. No other treatment of Faraday has made so apparent the multiple interrelations of his various research subjects—chemical, electrical, metallurgical, pneumatic, acoustical, and others. Nor has any other author so consistently attempted the imaginative reconstructions, essential if Faraday is to be understood, of the conceptual and experimental processes which led to his epochal discoveries, e.g. electromagnetic induction, magneto-optic effects, diamagnetism, and the concept of the electric and magnetic field. The very persistence with which Williams pursues these goals would itself insure the value of his book as a guide to the development of Faraday's science. Williams intends more than a guide, however, and his detailed analyses frequently do not withstand the scrutiny they invite.

The most easily recognised of the difficulties occur in passages which reconstruct the historical context—scientific concepts and problems—within which Faraday's research was done. For anyone, biographer or not, who aims to illuminate scientific development, the attempt is essential. Williams knows this and has read widely in nineteenth-century scientific literature, but his execution is often clumsy nevertheless. For example, in presenting the problems which Faraday's electrochemical researches aimed to resolve, he attributes to Ritter (pp. 228 f.) an experiment in which two water-filled electrolytic cells, connected in series, evolve gases only at the two outermost electrodes, the ones connected directly to the battery. In the context of electrochemical theory before Faraday, Williams supposes, this experiment implied that hydrogen, if truly an element, 'could travel *through* the wire between the [two electrolytic] tubes'. Escape from that interpretation was, he indicates, among the problems Faraday would have to solve. Somehow, Williams has failed to notice that the experiment, as described, presents the same challenge to modern theory that it is supposed to have presented to Faraday. That must be why he did not check Ritter's article once more and discover that the relevant experiments there described are different in a vital respect from the one he reports—one electrode in each cell is surrounded by acid. Or, if the organisation of Williams's narrative were tighter, he might have been led back to Ritter when attempting to describe Faraday's explanation of the putative effect. But, having once posed the gratuitous challenge, he never returns to it.

In its purity that example is far from typical. I doubt that a fully comparable one could be found in the book. But the careless reporting, organisational loose ends, and what occasionally looks like an insufficiently immediate grasp of relevant scientific theories do recur, though usually singly and with more equivocal effects. For example, Williams describes Volta's discovery of the electric current (p. 56) as follows: 'In 1794 he showed that the mere contact of two dissimilar metals could produce an electrostatic charge; when the metallic contact was supplemented by the intervention of a fluid conductor, a continuous circulation of the electric fluid resulted (see Figure I).' Apparently readers are to think of the liquid as placed *between* the two plates of dissimilar metal; that interpretation is enforced by the diagram to which Williams refers.[1] Today's science students do, of course, think of Volta's work in that way, but the view is not that of Volta, of Biot, or of the major Continental school which they inaugurated. For these men the unit cell was the two unlike pieces of metal in direct contact. Liquids intervened only as series conductors between bimetallic cells. The use of liquids was thought to eliminate the electrostatic tensions (we should call them contact potentials) which metallic conductors would have evoked. As an integral part of the electrostatic interpretation of electric-current phenomena the difference is consequential. The electrostatic view made the pile a self-charging battery of Leyden jars, treated external conductors as mere discharge routes, and analysed resistance in terms suggested by Coulomb for the very different phenomena we should call leakage.[1]

---

[1] An additional difficulty is illustrated here. Williams's diagrams are sometimes obscure, have no legends, and are insufficiently keyed to text. Only while preparing this review have I realised that the solid black portion of the diagram on p. 56 must be the liquid. If that symbolic coding is transferred to the diagram of the pile on p. 57, then the drawing does not correspond to Volta's. If, as I suppose, solid white is liquid, then the pile is not made up by stacking the individual cells shown on the preceding page. Elsewhere in the book are essential diagrams that neither I nor my students have yet been able to decipher, e.g. those on pp. 152 and 425 as well as the vital redrawn Faraday sketch on p. 157.

That view of electric current phenomena, dominant in many circles until 1820, was still very influential during the years of Faraday's first research in electricity. But it was also under vehement attack from Ampère and a group which rapidly assembled around him. They introduced a radical distinction between electrostatic and current phenomena. The latter were to be investigated exclusively with the galvanometer; electrostatic tension was irrelevant (Ampère emphasised that it dropped to zero as soon as the circuit was closed); the interaction between current-carrying wires was of a different kind from that between static charges. Faraday knew the French literature in which the battle was joined; he corresponded with Ampère who, after Faraday and Davy, is the third major scientist discussed in Williams's book. Yet neither the electrostatic nor the Ampèrian position on these central issues is ever described or discussed. That must be part of the reason Williams can attribute Ohm's law to Faraday (see below) and elsewhere (pp. 224 f., n. 12) entirely miss the major sources of opposition to that law. Accepting it meant the abandonment of one vastly influential aspect of Ampère's work and a retreat to a view more like the Volta-Biot theory which Ampère had, for many scientists, displaced. The same failure of perception enables Williams (pp. 173 f.) to attribute to Ampère an electrostatic explanation of the interaction between currents which, though undoubtedly important to Faraday's discovery of induction, was foreign both to Ampère and the pre-Weberian members of his school. Similar difficulties are, I suspect, to be found in Williams's discussion of the relation between Faraday's electrochemical researches and the earlier work of Grotthus and Davy, but there is no room to develop that point here.

Shortcomings of this sort are, of course, less frequent when Williams deals with Faraday himself as he does in much of the book. But they do occur repeatedly, often in substantive relation to the Whiggish misconstructions illustrated above. For example, Williams discusses (pp. 209 f.) experiments and texts from which he concludes: 'What Faraday had stated was clearly Ohm's law. ... Had Faraday taken the one final step of putting his views into the equation I = E/R, he would have provided an enormous stimulus to the progress of electrical science.' But the introduction of 'electromotive *force*' (Williams's term for *E* in the preceding equation) is gratuitous. Faraday says only that in electromagnetic induction, 'the tendency to generate a current must be directly as [the conducting] power'. In context the passage is plausibly reading as evidence that Faraday thought the 'tendency to produce a current' depended on the nature of the material in which current was induced, an interpretation which gains force from its relation to Faraday's views on the electrotonic state. Nor does Williams pause to wonder whether 'conductibility', as it occurs in this discussion, is at all the notion that develops after the assimilation of Ohm's law though he presents evidence for the continued currency of the leakage concept when discussing Ritchie's experiments three pages later. Whether or not my own conjectural reading is correct, what Williams has given us is neither Faraday's thought nor its context in the contemporary research of others.

---

[1] For documentation of a number of these points and for the central idea in the remarks on Ampère, below, I am indebted to a still unpublished paper by Mr Theodore M. Brown. I am grateful to him for permission to anticipate his publication on a few points originally developed in my Princeton University seminar.

Elsewhere Williams reverses the pattern, giving us Faraday's thought so literally and uncritically that its relation to subsequent electrical research is obscured. An important example is the description (pp. 297 f.) of the electrical 'shadow' experiment with which Faraday thought he had refuted Poisson's 'Newtonian' theory of electrostatic induction and established his own polarisation theory in its place. Williams does not inform his readers, and may conceivably be unaware, that Faraday is here dealing with just the sort of phenomenon for which Poisson's theory was devised and with which it was brilliantly successful. As a result, one is deprived of an opportunity to see how Faraday's well-known inability to grasp even the structure of a mathematical argument affected the details of his science. (Williams also obscures this vitally important facet of Faraday's relation with Ampère.) More important, no place is left for Kelvin's proof of the equivalence between Poisson's theory and a modified version of Faraday's though this is the point from which Maxwell started. In the section which follows (pp. 299-315) Williams struggles to eliminate the apparent contradiction, much discussed at the time, between Faraday's insistence that all electrostatic phenomena are ultimately polarisation effects and his use of 'free electricity' to explain how inductive states come into being (p. 313). What Williams has to say on this subject is necessarily speculative—he carefully labels it as such—and way well explain why Faraday never quite confronted the problem squarely. But the attempt to impose consistency deprives the problem of its historical position. The conflict between polarisation and free-charge explanations is deep and consequential. Maxwell inherited it from Faraday; Hertz underlined it in his critique of Maxwell; their successors wrestled with it to the end of the century. Readers might be fascinated to observe its early emergence.

These are, of course, all isolated difficulties torn from their context in a developmental discussion characterised by a richness and scope which my critical examples do not even suggest. Though many others could be given, it would remain the case that Williams must, more often than not, have interpreted Faraday correctly. Nevertheless, until much of this work is done again—a task which the existence of this biography will make far easier—it will remain impossible to tell which points are which. Williams is not a reliable guide to the fine-structure of Faraday's thought, much less to that of nineteenth-century science. And what holds for fine-structure, where texts are present, must hold as well for Williams's explicitly speculative theses about Faraday's main guiding hypotheses. According to Williams, Faraday was committed, throughout most of his productive research career, both to central tenets of German *Naturphilosophie* and to the Boscovichean atom. The former, at least is now increasingly recognised as a vital formative element in nineteenth-century scientific thought. The notion that the two played an essential role in Faraday's development is interesting and might even be correct. But Williams's evidence is extraordinarily thin, and it is far from apparent that any speculations so far-reaching are required for an understanding of Faraday. At the moment they serve to block more obvious lines of research.

The structure of Williams's main argument for Faraday's debt to *Naturphilosophie* is *post hoc ergo propter hoc.* The *Naturphilosophen*, first, and Faraday, second, rejected subtle fluids in favour of natural forces and were further convinced that the various forces of nature were convertible into one another. Those theses were not, however, either in isolation or combination, the exclusive possession of the *Naturphilosophen.* The first had never been lost from view after Newton, and the second had existed at times and places, e.g. eighteenth-century France, where the influence of *Naturphilosophie* is hard to credit. Though I have no idea where Faraday came upon them or whether he invented them for himself, the very limited parallel to *Naturphilosophie* is dubious evidence of influence. And the literary chain by which Williams suggests Faraday may have acquired them seems even weaker. Often it depends upon equating *Naturphilosophie* with Kant's philosophy and alluding to the undoubtedly considerable influence of Kant. The key concept of the interconvertibility of forces is not, however, to be found in Kant, not even in his closest approach to *Naturphilosophie*, the influential *Metaphysische Anfangsgründe der Naturwissenschaft.* No chain connecting Faraday to Kant can have supplied the former with the idea. *Naturphilosophie* itself could have been a source, but there is only one reference under this heading in Williams's index, and he has almost nothing to say about the philosophers who, after Kant, developed and established that view. His most helpful suggestion is a route via Coleridge to Davy to Faraday. But Davy, though he rejected subtle fluids, was not apparently a believer in convertibility. More careful discussion of this whole subject is essential.

Dealing with the Boscovichean atom, Williams feels himself on firmer ground, for in 1844 Faraday did write that 'the atoms of Boscovich appear to me to have a great advantage over the more usual notion' (p. 378). Williams supposes that Faraday had, in fact, embraced the point-atom years earlier—in this case there is no problem of transmission—and deployed it consequentially in his research. His failure to mention the belief before 1844 is, Williams believes, due to his fear of displaying metaphysical commitment. In tracing Faraday's thought, Williams again and again shows how a Boscovichean would have analysed one of Faraday's experiments or presented one of his arguments and then imputes similar thought processes to Faraday.

Here, too, Williams may be right, but I find his arguments even less convincing (partly because they seem so gratuitous—it is not clear that there are profound puzzles in this area) than those he advances for the influence of *Naturphilosophie.* Fear of public metaphysical commitment can scarcely explain the absence of point-atoms from Faraday's *Diary.* In any case, the Boscovichean atom, thoroughly Newtonian in spirit, was not anathema to British scientists as *Naturphilosophie* often was. Faraday's commitment, if significant to his research, should have left more literary traces. More important, the reiterated argument that particular published texts of Faraday's disclose his use of the point-atom depend upon the implicit premise that the only other view he could have held was a crude Daltonianism. (on p. 129 Williams speaks of Faraday's '*experimentum crucis* which could decide between the Daltonian and Boscovichean theories', and compare pp. 123 and 245). On one occasion (p. 251) Williams even appears to attribute 'the coolness of the reception given Faraday's theoretical concepts' to the fact that many chemists were 'still visualizing chemical combination in terms of hooks and eyes'. Williams ought not be held to his catch phrase, but the words are nonetheless symptomatic of a level of argument that does no justice to the elaboration of chemical concepts during the eighteenth and early nineteenth centuries. Repeatedly Williams argues for Boscovichean atomism from passages in which Faraday would seem to have been using more standard chemical ideas. One particularly clear-cut example occurs on pages 278-80. Williams's excellent index, under 'Atomism: Boscovichean', will lead to a number of others.

Possibly these difficulties and others in the analytic passages that bulk so large in Williams's narrative are an inevitable concomitant of the attempt to assault a whole new area of scholarship. In any case, no one can regret the overall result of the attempt. Williams has opened up a vast range of new problems to scholars, has supplied quantities of data for their use, and has proliferated imaginative connections for them to confirm or replace. But, both in its methods and conclusions, the book is an opening up of territory, not a conquest. Unless that is recognised, Williams's *Faraday* could do more harm than good.

THOMAS S. KUHN
Princeton University
Princeton, N.J., U.S.A.